III. *The Bakerian Lecture. An Account of some new analytical Researches on the Nature of certain Bodies, particularly the Alkalies, Phosphorus, Sulphur, Carbonaceous Matter, and the Acids hitherto undecompounded; with some general Observations on Chemical Theory.*

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1. *Introduction.*

In the following pages, I shall do myself the honour of laying before the Royal Society, an account of the results of the different experiments, made with the hopes of extending our knowledge of the principles of bodies by the new powers and methods arising from the applications of electricity to chemistry, some of which have been long in progress, and others of which have been instituted since their last session.

The objects which have principally occupied my attention, are the elementary matter of ammonia, the nature of phosphorus, sulphur, charcoal, and the diamond, and the constituents of the boracic, fluoric, and muriatic acids.

Amongst the numerous processes of decomposition, which I have attempted, many have been successful; and from those which have failed, some new phenomena have usually resulted which may possibly serve as guides in future inquiries. On this account, I shall keep back no part of the investigation, and I shall trust to the candour of the Society for an excuse for its imperfection.

The more approaches are made in chemical inquiries towards the refined analysis of bodies, the greater are the obstacles which present themselves, and the less perfect the results.

All the difficulties which occur in analysing a body, are direct proofs of the energy of attraction of its constituent parts. In the play of affinities with respect to secondary compounds even, it rarely occurs that any perfectly pure or unmixed substance is obtained; and the principle applies still more strongly to primary combinations.

The first methods of experimenting on new objects likewise are necessarily imperfect; novel instruments are demanded, the use of which is only gradually acquired, and a number of experiments of the same kind must be made, before one is obtained from which correct data for conclusions can be drawn.

2. *Experiments on the Action of Potassium on Ammonia, and Observations on the Nature of these two Bodies.*

In the Bakerian lecture, which I had the honour of reading before the Society, November 19, 1807, I mentioned that in heating potassium strongly in ammonia, I found that there was a considerable increase of volume of the gas, that hydrogene and nitrogene were produced, and that the potassium appeared to be oxidated; but this experiment, as I had not been able to examine the residuum with accuracy, I did not publish. I stated it as an evidence, which I intended to pursue more fully, of the existence of oxygene in ammonia.

In a paper read before the Royal Society last June, which they have done me the honour of printing, I have given an

account of various experiments on the amalgam from ammonia, discovered by Messrs. BERZELIUS and PONTIN, and in a note attached to this communication, I ventured to controvert an opinion of M. M. GAY LUSSAC and THENARD, with respect to the agency of potassium and ammonia, even on their own statement of facts, as detailed in the Moniteur for May 27, 1808.

The general obscurity belonging to these refined objects of research, their importance and connection with the whole of chemical theory, have induced me since that time to apply to them no inconsiderable degree of labour and attention; and the results of my inquiries will, I trust, be found not only to confirm my former conclusions; but likewise to offer some novel views.

In the first of these series of operations on the action of potassium on ammonia, I used retorts of green glass; I then suspecting oxygene might be derived from the metallic oxides in the green glass, employed retorts of plate glass, and last of all, I fastened the potassium upon trays of platina, or iron, which were introduced into the glass retorts furnished with stop cocks. These retorts were exhausted by an excellent air pump, they were filled with hydrogene, exhausted a second time, and then filled with ammonia from an appropriate mercurial gas holder.* In this way the gas was operated upon in a high degree of purity, which was always ascertained; and all the operations performed out of the contact of mercury, water, or any substances that could interfere with the results.

I at first employed potassium procured by electricity; but

* A representation of the instruments and apparatus is annexed.

I soon substituted for it the metal obtained by the action of ignited iron upon potash, in the happy method discovered by M. M. GAY LUSSAC and THENARD, finding that it gave the same results, and could be obtained of an uniform quality,* and in infinitely larger quantities, and with much less labour and expense.

When ammonia is brought in contact with about twice its weight of potassium at common temperatures, the metal loses its lustre and becomes white, there is a slight diminution in the volume of the gas; but no other effects are produced. The white crust examined proves to be potash, and the ammonia is found to contain a small quantity of hydrogene, usually not more than equal in volume to the metal. On heating the potassium in the gas, by means of a spirit lamp applied to the bottom of the retort, the colour of the crust is seen to change from white to a bright azure, and this gradually passes through shades of bright blue and green into dark olive. The crust and the metal then fuse together; there is a considerable effervescence, and the crust passing off to the sides, suffers the brilliant surface of the potassium to appear. When the potassium is cooled in this state it is again covered with the white crust. By heating a second time, it swells considerably, becomes porous, and appears crystallized, and of a beautiful

* When the potash used for procuring potassium in this operation was very pure, and the iron turnings likewise very pure and clean, and the whole apparatus free from any foreign matters, the metal produced differed very little, in its properties, from that obtained by the VOLTAIC battery. Its lustre, ductility, and inflammability were similar. Its point of fusion and specific gravity were, however, a little higher it requiring nearly 130° of FAHRENHEIT to render it perfectly fluid, and being to water as 7960 to 10000, at 60° FAHRENHEIT. This I am inclined to attribute to its containing a minute proportion of iron.

azure tint; the same series of phenomena, as those before described, occur in a continuation of the process, and it is finally entirely converted into the dark olive coloured substance.

In this operation, as has been stated by M. M. Gay Lussac and Thenard, a gas which gives the same diminution by detonation with oxygene, as hydrogene is evolved, and ammonia disappears.

The proportion of the ammonia which looses its elastic form, as I have found by numerous trials, varies according as the gas employed contains more or less moisture.

Thus eight grains of potassium, during its conversion into the olive coloured substance, in ammonia saturated with water at 63° Fahrenheit, and under a pressure equal to that of 29.8 inches of mercury, had caused the disappearance of twelve cubical inches and a half of ammonia; but the same quantity of metal acted upon under similar circumstances, except that the ammonia had been deprived of as much moisture as possible by exposure for two days to potash that had been ignited, occasioned a disappearance of sixteen cubical inches of the volatile alkali.

Whatever be the degree of moisture of the gas, the quantities of inflammable gas generated have always appeared to me to be equal for equal quantities of metal. M. M. Gay Lussac and Thenard are said to have stated, that the proportions in their experiment were the same as would have resulted from the action of water upon potassium. In my trials, they have been rather less. Thus, in an experiment conducted with every possible attention to accuracy of manipulation, eight grains of potassium generated, by their operation upon water,

eight cubical inches and a half of hydrogene gas: and eight grains from the same mass, by their action upon ammonia, produced eight cubical inches and one eighth of inflammable gas. This difference is inconsiderable, yet I have always found it to exist, even in cases where the ammonia has been in great excess, and every part of the metal apparently converted into the olive coloured substance.

No other account of the experiments of M. M. GAY LUSSAC and THENARD has, I believe, as yet been received in this country, except that in the Moniteur already referred to; and in this no mention is made of the properties of the substance produced by the action of ammonia on potassium. Having examined them minutely and found them curious, I shall generally describe them.

1. It is crystallized and presents irregular facets, which are extremely dark, and in colour and lustre not unlike the protoxide of iron; it is opaque when examined in large masses, but is semi-transparent in thin films, and appears of a bright brown colour by transmitted light.

2. It is fusible at a heat a little above that of boiling water, and if heated much higher, emits globules of gas.

3. It appears to be considerably heavier than water, for it sinks rapidly in oil of sassafras.

4. It is a non-conductor of electricity.

5. When it is melted in oxygene gas, it burns with great vividness, emitting bright sparks. Oxygene is absorbed, nitrogene is emitted, and potash, which from its great fusibility seems to contain water, is formed.

6. When brought in contact with water, it acts upon it with much energy, produces heat, and often inflammation, and

evolves ammonia. When thrown upon water, it disappears with a hissing noise, and globules from it often move in a state of ignition upon the surface of the water. It rapidly effervesces and deliquesces in air, but can be preserved under naphtha, in which, however, it softens slowly, and seems partially to dissolve. When it is plunged under water filling an inverted jar, by means of a proper tube, it disappears instantly with effervescence, and the non-absorbable elastic fluid liberated is found to be hydrogene gas.

By far the greatest part of the ponderable matter of the ammonia, that disappears in the experiment of its action upon potassium, evidently exists in the dark fusible product. On weighing a tray containing six grains of potassium, before and after the process, the volatile alkali employed having been very dry, I found that it had increased more than two grains; the rapidity with which the product acts upon moisture, prevented me from determining the point with great minuteness; but I doubt not, that the weight of the olive coloured substance and of the hydrogene disengaged precisely equals the weight of the potassium, and ammonia consumed.

M. M. Gay Lussac and Thenard* are said to have procured from the fusible substance, by the application of a strong heat, two fifths of the quantity of ammonia that had disappeared in their first process, and a quantity of hydrogene and nitrogene in the proportions in which they exist in ammonia, equal to one fifth more.

* No notice is taken of the apparatus used by M. M. Gay Lussac and Thenard in the Moniteur; but, from the tenour of the details, it seems that they must have operated in glass vessels in the way heretofore adopted over mercury.

My results have been very different, and the reasons will, I trust, be immediately obvious.

When the retort containing the fusible substance is exhausted, filled with hydrogene and exhausted a second time, and heat gradually applied, the substance soon fuses, effervesces, and, as the heat increases, gives off a considerable quantity of elastic fluid, and becomes at length, when the temperature approaches nearly to dull redness, a dark gray solid, which, by a continuance of this degree of heat, does not undergo any alteration.

In an experiment, in which eight grains of potassium had absorbed sixteen cubical inches of well dried ammonia in a glass retort, the fusible substance gave off twelve cubical inches and half of gas, by being heated nearly to redness, and this gas analysed, was found to consist of three quarters of a cubical inch of ammonia, and the remainder of elastic fluids, which when mixed with oxygene gas in the proportion of $6\frac{1}{2}$ to 6, and acted upon by the electric spark diminished to $5\frac{1}{2}$. The temperature of the atmosphere, in this process, was 57° FAHRENHEIT, and the pressure equalled that of 30.1 inches of mercury.

In a similar experiment, in which the platina tray containing the fusible substance was heated in a polished iron tube, filled with hydrogene gas, and connected with a pneumatic apparatus containing very dry mercury, the quantity of elastic fluid given off all the corrections being made, equalled thirteen cubical inches and three quarters, and of these a cubical inch was ammonia; and the residual gas, and the gas introduced into the tube being accounted for, it appeared that the elastic fluid generated, destructible by detonation with

oxygene, was to the indestructible elastic fluid, as 2.5 to 1.

In this process, the heat applied approached to the dull red heat. The mercury, in the thermometer, stood at 62° FAHRENHEIT, and that in the barometer at 30.3 inches.

In various experiments on different quantities of the fusible substance, in some of which the heat was applied to the tray in the green glass retort, and in others, after it had been introduced into the iron tube; and in which the temperature was sometimes raised slowly and sometimes quickly, the comparative results were so near these that I have detailed, as to render any statement of them superfluous.

A little more ammonia, and rather a larger proportion of inflammable gas,* were in all instances evolved when the iron tube was used, which I am inclined to attribute to the following circumstances. When the tray was brought through the atmosphere to be introduced into the iron tube, the fusible substance absorbed a small quantity of moisture from the air, which is connected with the production of ammonia. And in the process of heating in the retort, the green glass was blackened, and I found that it contained a very small quantity of the oxides of lead and iron, which must have caused the disappearance of a small quantity of hydrogene.

M. M. GAY LUSSAC and THENARD, it appears from the statement, had brought the fusible substance in contact with mercury, which must have given to it some moisture; and when ever this is the case, it furnishes by heat variable quantities

* The average of six experiments made in a tube of iron, is 2.4 of inflammable gas to 1 of uninflammable. The average of three made in green glass retorts, is 2.3 to 1.

of ammonia. In one instance, in which I heated the fusible substance from nine grains of potassium, in a retort that had been filled with mercury in its common state of dryness, I obtained seven cubical inches of ammonia as the first product; and in another experiment which had been made with eight grains, and in which moisture was purposely introduced, I obtained nearly nine cubical inches of ammonia, and only four of the mixed gases.

I am inclined to believe, that if moisture could be introduced only in the proper proportion, the quantity of ammonia generated, would be exactly equal to that which disappeared in the first process.

This idea is confirmed by the trials which I have made, by heating the fusible substance with potash, containing its water of crystallization, and muriate of lime partially dried.*

In both these cases, ammonia was generated with great rapidity, and no other gas, but a minute quantity of inflammable gas, evolved, which was condensed by detonation with oxygene with the same phenomena as pure hydrogene.

In one instance, in which thirteen cubical inches of ammonia had disappeared, I obtained nearly eleven and three quarters by the agency of the water of the potash; the quantity of inflammable gas generated, was less than four tenths of a cubical inch.

In another, in which fourteen cubical inches had been

* If water, in its common form, is brought in contact with the fusible substance, it is impossible to regulate the quantity, so as to gain conclusive results, and a very slight *excess* of water causes the disappearance of a very large quantity of the ammonia generated. In potash and muriate of lime, in certain states of dryness, the water is too strongly attracted by the saline matter to be given off, except for the purpose of *generating* the ammonia.

absorbed, I procured by the operation of the moisture of muriate of lime, nearly eleven cubical inches of volatile alkali, and half a cubical inch of inflammable gas; and the differences, there is every reason to believe, were owing to an excess of water in the salts, by which some of the gas was absorbed.

Whenever, in experiments on the fusible substance, it has been procured from ammonia saturated with moisture, I have always found that more ammonia is generated from it by mere heat; and the general tenour of the experiments incline me to believe, that the small quantity, produced in experiments performed in vacuo, is owing to the small quantity of moisture furnished by the hydrogene gas introduced, and that the fusible substance, heated out of the presence of moisture, is incapable of producing volatile alkali.

M. M. Gay Lussac and Thenard, it is stated, after having obtained three fifths of the ammonia or its elements that had disappeared in their experiment, by heating the product; procured the remaining two fifths, by adding water to the residuum, which after this operation was found to be potash. No notice is taken of the properties of this residuum, which as the details seem to relate to a single experiment, probably was not examined; nor as moisture was present at the beginning of their operations could any accurate knowledge of its nature have been gained.

I have made the residuum of the fusible substance after it has been exposed to a dull red heat, out of the contact of moisture, an object of particular study, and I shall detail its general properties.

It was examined under naphtha, as it is instantly destroyed by the contact of air.

1. Its colour is black, and its lustre not much inferior to that of plumbago.

2. It is opaque even in the thinnest films.

3. It is very brittle, and affords a deep gray powder.

4. It is a conductor of electricity.

5. It does not fuse at a low red heat, and when raised to this temperature, in contact with plate glass, it blackens the glass, and a grayish sublimate rises from it, which likewise blackens the glass.

6. When exposed to air at common temperatures, it usually takes fire immediately, and burns with a deep red light.

7. When it is acted upon by water, it heats, effervesces most violently, and evolves volatile alkali, leaving behind nothing but potash. When the process is conducted under water, a little inflammable gas is found to be generated. A residuum of eight grains giving in all cases about $\frac{20}{100}$ of a cubical inch.

8. It has no action upon quicksilver.

9. It combines with sulphur and phosphorus by heat, without any vividness of effect, and the compounds are highly inflammable, and emit ammonia, and the one phosphuretted and the other sulphuretted hydrogene gas, by the action of water.

As an inflammable gas alone, having the obvious properties of hydrogene is given off during the action of potassium upon ammonia, and as nothing but gases apparently the same as hydrogene and nitrogene, nearly in the proportions in which they exist in volatile alkali, are evolved during the exposure of the compound to the degree of heat which I have specified; and as the residual substance produces ammonia with a little hydrogene by the action of water, it occurred to

me, that, on the principles of the antiphlogistic theory, it ought to be a compound of potassium, a little oxygene and nitrogene, or a combination of a suboxide of potassium and nitrogene; for the hydrogene disengaged in the operations of which it was the result, nearly equalled the whole quantity contained in the ammonia employed; and it was easy to explain the fact of the reproduction of the ammonia by water, on the supposition, that by combination with one portion of the oxygene of the water, the oxide of potassium became potash, and by combination with another portion and its hydrogene, the nitrogene was converted into volatile alkali.

With a view to ascertain this point, I made several experiments on various residuums, procured in the way that I have just stated, from the action of equal quantities of potassium on dry ammonia in platina trays, each portion of metal equalling six grains.

In the first trials, I endeavoured to ascertain the quantity of ammonia generated by the action of water upon a residuum, by heating it with muriate of lime or potash partially deprived of moisture; and after several trials, many of which failed, I succeeded in obtaining four cubical inches and a half of ammonia. In three other cases, where there was reason to suspect a small excess of water, the quantities of ammonia were three cubical inches and a half, three and eight tenths, and four and two tenths.

These experiments were performed in the iron tube used for the former process; the tray was not withdrawn; but the salt introduced in powder, and the apparatus exhausted as before, then filled with hydrogene, and then gently heated in a small portable forge.

Having ascertained what quantity of ammonia was given off from the residuum, I endeavoured to discover what quantity of nitrogene it produced in combustion, and what quantity of oxygene it absorbed. The methods that I employed, were by introducing the trays into vessels filled with oxygene gas over mercury. The product often inflamed spontaneously, and could always be made to burn by a slight degree of heat.

In the trial that I regard as the most accurate, two cubical inches and a half of oxygene were absorbed, and only a cubical inch and one tenth of nitrogene evolved.

Surprised at the smallness of the quantity of the nitrogene, I sought for ammonia in the products of these operations; but various trials convinced me that none was formed. I examined the solid substances produced, expecting nitrous acid; but the matter proved to be dry potash, apparently pure, and not affording the slightest traces of acid.

The quantity of nitrogene existing in the ammonia, which this residuum would have produced by the action of *water*, supposing the volatile alkali decomposed by electricity, would have equalled at least two cubical inches and a quarter.

I heated the same proportions of residuum with the red oxide of mercury, and the red oxide of lead in vacuo, expecting that when oxygene was supplied in a gradual way, the result might be different from that of combustion; but in neither of these cases did the quantity of nitrogene exceed a cubical inch and a half.

But on what could this loss of nitrogene depend; had it entered into any unknown form with oxygene, or did it not really exist in the residuum in the same quantity, as in the *ammonia* produced from it?

I hoped that an experiment of exposing the residuum to intense heat might enlighten the inquiry. I distilled one of the portions which had been covered with naphtha, in a tube of wrought platina made for the purpose. The tube had been exhausted and filled with hydrogene, and exhausted again, and was then connected with a pneumatic mercurial apparatus. Heat was at first slowly applied till the naphtha had been driven over. It was then raised rapidly by an excellent forge. When the tube became cherry red, gas was develloped; it continued to be generated for some minutes. When the tube had received the most intense heat that could be applied, the operation was stopped. The quantity of gas collected, making the proper corrections and reductions, would have been three cubical inches and a half at the mean temperature and pressure. Twelve measures of it were mixed with six of oxygene gas, the electrical spark was passed through the mixture; a strong inflammation took place, the diminution was to three measures and a half, and the residuum contained oxygene. This experiment was repeated upon different quantities with the same comparative results.

In examining the platina tube, which had a screw adapted to it at the lower extremity, by means of which it could be opened. The lower part was found to contain potash, which had all the properties of the pure alkali, and in the upper part there was a quantity of potassium. Water poured into the tube, produced a violent heat and inflammation; but no smell of ammonia.

This result was so unexpected and so extraordinary, that I at first supposed there was some source of error. I had calculated upon procuring nitrogene as the only aeriform

product; I obtained an elastic fluid which gave much more diminution by detonation with oxygene, than that produced from ammonia by electricity.

I now made the experiment, by heating the entire fusible substance, from six grains of potassium which had absorbed twelve cubical inches of ammonia, in the iron tube, in the manner before described. The heat was gradually raised to whiteness, and the gas collected in two portions. The whole quantity generated, making the usual corrections for temperature and pressure, and the portion of hydrogene originally in the tube, and the residuum, would have been fourteen cubical inches and a half at the mean degree of the barometer and thermometer. Of these, nearly a cubical inch was ammonia and the remainder a gas, of which the portion destructible by detonation with oxygene, was to the indestructible portion, as 2.7 to 1.

The lower part of the tube, where the heat had been intense, was found surrounded with potash in a vitreous form; the upper part contained a considerable quantity of potassium.

In another similar experiment, made expressly for the purposes of ascertaining the quantity of potassium recovered, the same elastic products were evolved. The tube was suffered to cool, the stop-cock being open in contact with mercury, it was filled with mercury, and the mercury displaced by water; when two cubical inches and three quarters of hydrogene gas were generated, which proved that at least two grains and a half of potassium had been revived.

Now, if a calculation be made upon the products in these operations, considering them as nitrogene and hydrogene, and taking the common standard of temperature and pressure, it

will be found, that by the decomposition of 11 cubical inches of ammonia equal to 2.05 grains, there is generated 3.6 cubical inches of nitrogene equal to 1.06 grains, and 9.9 cubical inches of hydrogene, which added to that disengaged in the first operation equal to about 6.1 cubical inches, are together equal to .382 grains; and the oxygene added to 3.5 grains of potassium would be .6 grains, and the whole amount is 2.04 grains; and 2.05 — 2.04 = .01. But the same quantity of ammonia, decomposed by electricity, would have given 5.5 cubical inches of nitrogene equal to 1.6 grains, and only 14 cubical inches of hydrogene* equal to .33, and allowing the separation of oxygene in this process in water, it cannot be estimated at more than .11 or .12.

So that if the analysis of ammonia by electricity at all approaches towards accuracy; in the process just described, there is a considerable loss of nitrogene, and a production of oxygene and inflammable gas.

And in the action of water upon the residuum, in the experiment page 52, there is an apparent generation of nitrogene.

How can these extraordinary results be explained?

The decomposition and composition of nitrogene seem proved, allowing the correctness of the data; and one of its elements appears to be oxygene; but what is its other elementary matter?

Is the gas that appears to possess the properties of hydrogene, a new species of inflammable aeriform substance?

Or has nitrogene a metallic basis which alloys with the iron or platina?

* See Phil. Trans. 1808, p. 40.

Or is water alike the *ponderable* matter of nitrogene, hydrogene, and oxygene?

Or is nitrogene a compound of hydrogene with a larger proportion of oxygene than exists in water?

These important questions, the two first of which seem the least likely to be answered in the affirmative, from the correspondence between the weight of the ammonia decomposed, and the products, supposing them to be known substances, I I shall use every effort to solve by new labours, and I hope soon to be able to communicate the results of further experiments on the subject to the Society.

As the inquiry now stands, it is however sufficiently demonstrative, that the opinion which I had ventured to form respecting the decomposition of ammonia in this experiment, is correct; and that M. M. Gay Lussac's and Thenard's idea of the decomposition of the potassium, and their theory of its being compounded of hydrogene and potash, are unfounded.

For a considerable part of the potassium is recovered unaltered, and in the entire decomposition of the fusible substance, there is only a small excess of hydrogene above that existing in the ammonia acted upon.

The mere phenomena of the process likewise, if minutely examined, prove the same thing.

After the first slight effervescence, owing to the water absorbed by the potash formed upon the potassium during its exposure to the air, the operation proceeds with the greatest tranquillity. No elastic fluid is given off from the potassium; it often appears covered with the olive coloured substance, and if it were evolving hydrogene; this must pass through

the fluid; but even to the end of the operation, no such appearance occurs.

The crystallized and spongy substance, formed in the first part of the process, I am inclined to consider as a combination of ammonium and potassium, for it emits a smell of ammonia when exposed to air, and is considerably lighter than potassium.

I at first thought that a solid compound of hydrogene and potassium might be generated in the first part of this operation: but experiments on the immediate action of potassium and hydrogene did not favour this opinion. Potassium, as I ventured to conclude in the Bakerian Lecture for 1807,* is very

* M. M. GAY LUSSAC and THENARD seem to be of a different opinion. In the Moniteur, to which I have so often referred, it is related, that these distinguished chemists, exposing hydrogene to potassium at a high temperature, found that the hydrogene was absorbed, and that it formed a compound with the potassium of a light gray colour, from which hydrogene was capable of being obtained by the action of water or mercury.

After a number of trials, I have not been able to witness this result. In an experiment which I made in the presence of Mr. PEPYS, and which I have often repeated, and twice before a numerous assembly, in retorts of plate glass, four grains of potassium were heated in fourteen cubical inches of pure hydrogene. At first, white fumes arose and precipitated themselves in the neck of the retort. When a considerable film of the precipitate had collected, its colour appeared a bright gray, and after the first two or three minutes, it ceased to be formed.

The bottom of the retort was heated to redness, when the potassium began to sublime and condense on the sides.

The process was stopped, and the retort suffered to cool. The absorption was not equal to a quarter of a cubical inch. When the retort was *broken*, the gas in passing into the atmosphere, produced an explosion with most vivid light, and white fumes. The potassium remaining in the retort, and that which had sublimed, seemed unaltered in their properties.

The grayish substance inflamed by the action of water, but did not seem to be combinable with mercury. I am inclined to attribute its formation to the agency of

soluble in hydrogene; but, under common circumstances, hydrogene does not seem to be absorbable by potassium.

moisture suspended in the hydrogene, and to consider it as a triple compound of potassium, oxygene, and hydrogene.

When potassium is heated in a gas containing hydrogene, and from $\frac{1}{15}$ to $\frac{1}{30}$ of common air, it is formed in greater quantities, and a crust of it covers the metal, and in the process there is an absorption both of hydrogene and oxygene. It is likewise produced in experiments on the generation of potassium by exposing potash to ignited iron, at the time (I believe) that common air is admitted, during the cooling of the tube.

It is non-conducting, inflames spontaneously in air, and produces potash and aqueous vapour by its combustion.

When potassium is heated in hydrogene in a flint glass retort, or even for a great length of time in a green glass retort, there is an absorption of the gas; but this is independent of the presence of potassium, and is owing to the action of the metallic oxides in the glass upon the hydrogene.

If a solid compound of hydrogene and potassium could be formed, we might expect its existence in the experiment with the gun barrel, in which potassium is exposed to hydrogene at almost every temperature; but the metal formed in this process, when proper precautions are taken to exclude carbonaceous matters, is uniform in its properties, and generates for equal quantities, equal proportions of hydrogene by the action of water.

The general phenomena of this operation, shew indeed that the solution of potassium in hydrogene is intimately connected with the general principle of the decomposition, and confirm my first idea of the action of the two bodies.

Hydrogene dissolves a large quantity of potassium by heat, but the greater portion is precipitated on cooling. The attractions which determine the chemical change, seem to be that of iron for oxygene, of iron for potassium, and of hydrogene for potassium; and in experiments, in which a very intense heat is used for the production of potassium by iron, I have often found, that the gas which comes over, though it has passed through a tube cooled by ice, inflames spontaneously in the atmosphere, and burns with a most brilliant light which is purple at the edges, and throws off a dense vapour containing potash.

Sodium appears to be almost insoluble in hydrogene, and this seems to be one reason why it cannot be obtained, except in very minute quantities, in the experiment with the gun barrel.

Sodium, though scarcely capable of being dissolved in hydrogene alone, seems to be

3. *Analytical Experiments on Sulphur.*

I have referred, on a former occasion,* to the experiments of Mr. Clayfield and of Mr. Berthollet, jun. which seemed to show that sulphur, in its common form, contained hydrogene. In considering the analytical powers of the Voltaic apparatus, it occurred to me, that though sulphur, from its being a non-conductor, could not be expected to yield its elements to the electrical attractions and repulsions of the opposite surfaces, yet that the intense heat, connected with the contact of these surfaces, might possibly effect some alteration in it, and tend to separate any elastic matter it might contain.

On this idea some experiments were instituted in 1807. A curved glass tube, having a platina wire hermetically sealed in its upper extremity, was filled with sulphur. The sulphur was melted over a spirit lamp; and a proper connection being made with the Voltaic apparatus of one hundred plates of six inches, in great activity, a contact was made in the sulphur by means of another platina wire. A most brilliant spark, which appeared orange coloured through the sulphur, was produced, and a minute portion of elastic fluid rose to the upper extremity of the tube. By a continuation of the

soluble in the compound of hydrogene and potassium. By exposing mixtures of potash and soda, to ignited iron I have obtained some very curious alloys; which, whether the potassium or the sodium was in excess, were fluid at common temperatures. The compound containing an excess of potassium was even lighter than potassium (probably from its fluidity). All these alloys were in the highest degree inflammable. When a globule of the fluid alloy was touched by a globule of mercury, they combined with a heat that singed the paper upon which the experiment was made, and formed, when cool, a solid so hard, as not to be cut by a knife.

* Bakerian Lecture, 1808, p. 16.

process for nearly an hour, a globule equal to about the tenth of an inch in diameter was obtained, which, when examined, was found to be sulphuretted hydrogene.

This result perfectly coincided with those which have been just mentioned; but as the sulphur that I had used was merely in its common state, and as the ingenious experiments of Dr. THOMSON have shewn that sulphur in certain forms may contain water, I did not venture, at that time, to form any conclusion upon the subject.

In the summer of the present year, I repeated the experiment with every precaution. The sulphur that I employed was Sicilian sulphur, that had been recently sublimed in a retort filled with nitrogene gas, and that had been kept hot till the moment that it was used. The power applied was that of the battery of five hundred double plates of six inches, highly charged. In this case the action was most intense, the heat strong, and the light extremely brilliant; the sulphur soon entered into ebullition, elastic matter was formed in great quantities, much of which was permanent; and the sulphur, from being of a pure yellow, became of a deep red brown tint.

The gas, as in the former instance, proved to be sulphuretted hydrogene. The platina wires were considerably acted upon; the sulphur, at its point of contact with them, had obtained the power of reddening moistened litmus paper.

I endeavoured to ascertain the quantity of sulphuretted hydrogene evolved in this way from a given quantity of sulphur, and for this purpose, I electrized a quantity equal to about two hundred grains in an apparatus of the kind I have just

described, and when the upper part of the tube was full of gas, I suffered it to pass into the atmosphere; so as to enable me to repeat the process.

When I operated in this way, there seemed to be no limit to the generation of elastic fluid, and in about two hours a quantity had been evolved, which amounted to more than five times the volume of the sulphur employed. From the circumstances of the experiment, the last portion only could be examined, and this proved to be sulphuretted hydrogene. Towards the end of the process, the sulphur became extremely difficult of fusion, and almost opaque, and when cooled and broken, was found of a dirty brown colour.

The experiments upon the union of sulphur and potassium, which I laid before the Society last year, prove that these bodies act upon each other with great energy, and that sulphuretted hydrogene is evolved in the process, with intense heat and light.

In heating potassium in contact with compound inflammable substances, such as resin, wax, camphor, and fixed oils in close vessels out of the contact of the air, I found that a violent inflammation was occasioned, that hydrocarbonate was evolved; and that when the compound was not in great excess, a substance was formed, spontaneously inflammable at common temperatures, the combustible materials of which were charcoal and potassium.

Here was a strong analogy between the action of these bodies, and sulphur on potassium. Their physical properties likewise resemble those of sulphur; for they agree in being non-conductors, whether fluid or solid, in being transparent when fluid, and semi-transparent when solid, and highly

refractive; their affections by electricity are likewise similar to those of sulphur; for the oily bodies give out hydrocarbonate by the agency of the VOLTAIC spark, and become brown, as if from the deposition of carbonaceous matter.

But the resinous and oily substances are compounds of a small quantity of hydrogene and oxygene, with a large quantity of a carbonaceous basis. The existence of hydrogene in sulphur is fully proved, and we have no right to consider a substance, which can be produced from it in such large quantities, merely as an accidental ingredient.

The oily substances in combustion, produce two or three times their weight of carbonic acid and some water; I endeavoured to ascertain whether water was formed in the combustion of sulphur in oxygene gas, dried by exposure to potash: but in this case sulphureous acid is produced in much larger quantities than sulphuric acid, and this last product is condensed with great difficulty. In cases, however, in which I have obtained, by applying artificial cold, a deposition of acid in the form of a film of dew in glass retorts out of the contact of the atmosphere, in which sulphur had been burned in oxygene gas hygrometrically dry, it has appeared to me less tenacious and lighter than the common sulphuric acid of commerce, which in the most concentrated form in which I have seen it, namely, at 1.855, gave abundance of hydrogene as well as sulphur, at the negative surface in the VOLTAIC circuit, and hence evidently contained water.

The reddening of the litmus paper, by sulphur that had been acted on by VOLTAIC electricity, might be ascribed to its containing some of the sulphuretted hydrogene formed in the process; but even the production of this gas, as will be im-

mediately seen, is an evidence of the existence of oxygene in sulphur.

In my early experiments on potassium, procured by electricity, I heated small globules of potassium in large quantities of sulphuretted hydrogene, and I found that sulphuret of potash was formed; but this might be owing to the water dissolved in the gas, and I ventured to draw no conclusion till I had tried the experiment in an unobjectionable manner.

I heated four grains of potassium in a retort of the capacity of twenty cubical inches; it had been filled after the usual processes of exhaustion with sulphuretted hydrogene, dried by means of muriate of lime that had been heated to whiteness; as soon as the potassium fused, white fumes were copiously emitted, and the potassium soon took fire, and burnt with a most brilliant flame, yellow in the centre and red towards the circumference.*

The diminution of the volume of the elastic matter, in this operation, did not equal more than two cubical inches and a half. A very small quantity of the residual gas only was absorbable by water. The non-absorbable gas was hydrogene, holding a minute quantity of sulphur in solution.

A yellow sublimate lined the upper part of the retort, which proved to be sulphur. The solid matter formed was red at the surface like sulphuret of potash, but in the interior it was dark gray, like sulphuret of potassium. The piece of the retort containing it was introduced into a jar inverted over mercury,

* In the Moniteur, May 27, 1808, in the account of M. M. GAY LUSSAC's and THENARD's experiments, it is mentioned, that potassium absorbs the sulphur and a part of the hydrogene of sulphuretted hydrogene; but the phenomena of inflammation is not mentioned, nor are the results described.

and acted upon by a small quantity of dense muriatic acid, diluted with an equal weight of water, when there were disengaged two cubical inches and a quarter of gas, which proved to be sulphuretted hydrogene.

In another experiment, in which eight grains of potassium were heated in a retort of the capacity of twenty cubical inches, containing about nineteen cubical inches of sulphuretted hydrogene, and a cubical inch of phosphuretted hydrogene, which was introduced for the purpose of absorbing the oxygene of the small quantity of common air admitted by the stop-cock, the inflammation took place as before, there was a similar precipitation of sulphur on the sides of the retort; the mass formed in the place of the potassium was orange externally, and of a dark gray colour internally, as in the last instance; and when acted on by a little water holding muriatic acid in solution, there were evolved from it five cubical inches only of sulphuretted hydrogene.

Both these experiments concur in proving the existence of a principle in sulphuretted hydrogene, capable of destroying partially the inflammability of potassium, and of producing upon it all the effects of oxygene; for had the potassium combined merely with pure combustible matter, it ought, as will be seen distinctly from what follows, to have evolved by the action of the acid, a volume of sulphuretted hydrogene, at least equal to that of the hydrogene, which an equal weight of uncombined potassium would have produced by its operation upon water.

Sulphuretted hydrogene, as has been long known to chemists, may be formed by heating sulphur strongly in hydrogene gas. I heated four grains of sulphur in a glass retort,

containing about twenty cubical inches of hydrogene, by means of a spirit lamp, and pushed the heat nearly to redness. There was no perceptible change of volume in the gas after the process; the sulphur that had sublimed was unaltered in its properties, and about three cubical inches of an elastic fluid absorbable by water were formed: the solution reddened litmus, and had all the properties of a solution of pure sulphuretted hydrogene. Now if we suppose sulphuretted hydrogene to be constituted by sulphur dissolved in its unaltered state in hydrogene, and allow the existence of oxygene in this gas; its existence must likewise be allowed in sulphur, for we have no right to assume that sulphur in sulphuretted hydrogene is combined with more oxygene than in its common form: it is well known, that when electrical sparks are passed through sulphuretted hydrogene, a considerable portion of sulphur is separated without any alteration in the volume of the gas. This experiment I have made more than once, and I found that the sulphur obtained, in fusibility, combustibility, and other sensible properties did not perceptibly differ from common sublimed sulphur.

According to these ideas, the intense ignition produced by the action of sulphur, on potassium and sodium, must not be ascribed merely to the affinity of the metals of the alkalies for its basis, but may be attributed likewise to the agency of the oxygene that it contains.

The minute examination of the circumstances of the action of potassium and sulphur likewise confirms these opinions.

When two grains of potassium and one of sulphur were heated gently in a green glass tube filled with hydrogene, and connected with a pneumatic apparatus, there was a most

intense ignition produced by the action of the two bodies, and one eighth of a cubical inch of gas was disengaged, which was sulphuretted hydrogene. The compound was exposed in a mercurial apparatus to the action of liquid muriatic acid; when a cubical inch and quarter of aeriform matter was produced, which proved to be pure sulphuretted hydrogene.

The same experiment was repeated, except that four grains of sulphur were employed instead of one. In this case, a quarter of a cubical inch of gas was disengaged during the process of combination; and when the compound was acted upon by muriatic acid, only three quarters of a cubical inch of sulphuretted hydrogene was obtained.

Now, *sulphuret* of potash produces sulphuretted hydrogene by the action of an acid; and if the sulphur had not contained oxygene, the hydrogene evolved by the action of the potassium in both these experiments ought to have equalled at least two cubical inches, and the whole quantity of sulphuretted hydrogene ought to have been more: and that so much less sulphuretted hydrogene was evolved in the second experiment, can only be ascribed to the larger quantity of oxygene furnished to the potassium by the larger quantity of the sulphur.

I have made several experiments of this kind with similar results. Whenever equal quantities of potassium were combined with unequal quantities of sulphur, and exposed afterwards to the action of muriatic acid, the largest quantity of sulphuretted hydrogene was furnished by the product containing the smallest proportion of sulphur, and in no case was the quantity of gas equal in volume to the quantity of hydrogene, which would have been produced by the mere action of potassium upon water.

From the general tenour of these various facts, it will not be, I trust, unreasonable to assume, that sulphur, in its common state, is a compound of small quantities of oxygene and hydrogene with a large quantity of a basis that produces the acids of sulphur in combustion, and which, on account of its strong attractions for other bodies, it will probably be very difficult to obtain in its pure form.

In metallic combinations even, it still probably retains its oxygene and part of its hydrogene. Metallic sulphurets can only be partially decomposed by heat, and the small quantity of sulphur evolved from them in this case when perfectly dry and out of the contact of air, as I found in an experiment on the sulphurets of copper and iron, exists in its common state, and acts upon potassium, and is affected by electricity in the same manner as native sulphur.

4. *Analytical Experiments on Phosphorus.*

The same analogies apply to phosphorus as to sulphur, and I have made a similar series of experiments on this inflammable substance.

Common electrical sparks, passed through phosphorus, did not evolve from it any permanent gas; but when it was acted upon by the VOLTAIC electricity of the battery of five hundred plates in the same manner as sulphur, gas was produced in considerable quantities, and the phosphorus became of a deep red brown colour, like phosphorus that has been inflamed and extinguished under water. The gas examined proved to be phosphuretted hydrogene, and in one experiment, continued for some hours, a quantity estimated to be nearly equal to four times the volume of the phosphorus employed was given

off. The light of the VOLTAIC spark in the phosphorus was at first a brilliant yellow, but as the colour of the phosphorus changed, it appeared orange.

I heated three grains of potassium in sixteen cubical inches of phosphuretted hydrogene; as soon as it was fused, the retort became filled with white fumes, and a reddish substance precipitated upon the sides and upper part of it. The heat was applied for some minutes. No inflammation took place.* When the retort was cool, the absorption was found to be less than a cubical inch. The potassium externally was of a deep brown colour, internally it was of a dull lead colour. The residual gas had lost its property of spontaneous inflammation, but seemed still to contain a small quantity of phosphorus in solution.

The phosphuret acted upon over mercury by solution of muriatic acid evolved only one cubical inch and three quarters of phosphuretted hydrogene.

From this experiment, there is great reason to suppose that phosphuretted hydrogene contains a minute proportion of oxygene, and consequently that phosphorus likewise may contain it; but the action of potassium on phosphorus itself furnishes perhaps more direct evidences of the circumstance.

One grain of potassium and one grain of phosphorus were fused together in a proper apparatus. They combined with the production of the most vivid light and intense ignition. During the process one tenth of a cubical inch of phosphuretted hydrogene was evolved. The phosphuret formed, exposed

* It is stated, in the account before referred to of M. M. GAY LUSSAC's and THENARD's experiments, that potassium inflames in phosphuretted hydrogene. My experiments upon this gas have been often repeated. I have never perceived any luminous appearance; but I have always operated in day-light.

to the action of diluted muriatic acid over mercury, produced exactly three tenths of a cubical inch of phosphuretted hydrogene.

In a second experiment, one grain of potassium was fused with three grains of phosphorus; in this case nearly a quarter of a cubical inch of phosphuretted hydrogene was generated during the ignition. But from the compound exposed to muriatic acid, only one tenth of a cubical inch could be procured.

Now it is not easy to refer the deficiency of phosphuretted hydrogene in the second case to any other cause than to the supply of oxygene to the potassium from the phosphorus; and the quantity of phosphuretted hydrogene evolved in the first case, is much less than could be expected, if both potassium and phosphorus consisted merely of pure combustible matter.

The phosphoric acid, formed by the combustion of phosphorus, though a crystalline solid, may still contain water. The hydrogene evolved from phosphorus by electricity proves indeed that this must be the case; and though the quantity of hydrogene and oxygene in phosphorus may be exceedingly small, yet they may be sufficient to give it peculiar characters; and till the basis is obtained free, we shall have no knowledge of the properties of the pure phosphoric element.

5. *On the States of the carbonaceous Principle in Plumbago, Charcoal, and the Diamond.*

The accurate researches of Messrs. ALLEN and PEPYS have distinctly proved, that plumbago, charcoal, and the diamond

produce very nearly the same quantities of carbonic acid, and absorb very nearly the same quantities of oxygene in combustion.

Hence it is evident, that they must consist principally of the same kind of elementary matter; but minute researches upon their chemical relations, when examined by new analytical methods will, I am inclined to believe, shew that the great difference in their physical properties does not merely depend upon the differences of the mechanical arrangement of their parts, but likewise upon differences in their intimate chemical nature.

I endeavoured to discover, whether any elastic matter could be obtained from plumbago very intensely ignited by the VOLTAIC battery in a Torricellian vacuum: but though the highest power of the battery of five hundred was employed, and though the heat was such, as in another experiment instantly melted platina wire of $\frac{1}{60}$th of an inch in diameter, yet no appearance of change took place upon the plumbago. Its characters remained wholly unaltered, and no permanent elastic fluid was formed.

I heated one grain of plumbago, with twice its weight of potassium, in a plate glass tube connected with a proper apparatus, and I heated an equal quantity of potassium alone in a tube of the same kind, for an equal length of time, namely, eight minutes. Both tubes were filled with hydrogene: no gas was evolved in either case. There was no ignition in the tube containing the plumbago, but it seemed gradually to combine with the potassium. The two results were exposed to the action of water; the result from the plumbago acted upon that fluid with as much energy as the other result, and

the two volumes of elastic fluids were 1.8 cubical inches and 1.9 cubical inches; and both gave the same diminution by detonation with oxygene, as pure hydrogene. Two grains of potassium, by acting upon water, would have produced two cubical inches and one eighth of hydrogene gas; the deficiency in the result, in which potassium alone was used, must be ascribed to the loss of a small quantity of metal, which must have been carried off in solution in the hydrogene, and perhaps, likewise, to the action of the minute quantity of metallic oxides in the plate glass. The difference in the quantity of hydrogene given off in the two results, is however too slight to ascribe it to the existence of oxygene in the plumbago.

I repeated this experiment several times with like results, and in two or three instances examined the compound formed. It was infusible at a red heat, had the lustre of plumbago. It inflamed spontaneously, when exposed to air, generated potash, and left a black powdery residuum. It effervesced most violently in water, and produced a gas, which burnt like pure hydrogene.

When small pieces of charcoal from the willow, that had been intensely ignited, were acted upon by VOLTAIC electricity in a Torricellian vacuum, every precaution being taken to exclude moisture from the mercury and the charcoal, the results were very different from those occuring in the case of plumbago.

When plumbago was used, after the first spark, which generally passed through a distance of about one eighth of an inch, there was no continuation of light, without a contact or an approach to the same distance; but from the charcoal a

flame seemed to issue of a most brilliant purple, and formed, as it were, a conducting chain of light of nearly an inch in length, at the same time that elastic matter was rapidly formed, some of which was permanent. After many unsuccessful trials, I at length succeeded in collecting the quantity of elastic fluid given out by half a grain of charcoal; the process had been continued nearly half an hour. The quantity of gas amounted to nearly an eighth of a cubical inch; it was inflammable by the electric spark with oxygene gas, and four measures of it absorbed three measures of oxygene, and produced one measure and a half of carbonic acid. The charcoal in this experiment had become harder at the point, and its lustre, where it had been heated to whiteness, approached to that of plumbago.

I heated two grains of potassium together with two grains of charcoal, for five minutes; and to estimate the effects of the metallic oxides and potash in the green glass tube, I made a comparative experiment, as in the case of plumbago; but there was no proof of any oxygene being furnished to the potassium from the charcoal in the process, for the compound acted upon water with great energy, and produced a quantity of inflammable gas, only inferior by one twelfth to that produced by the potassium, which had not been combined with charcoal, and which gave the same diminution by detonation with oxygene; and the slight difference may be well ascribed to the influence of foreign matters in the charcoal. There was no ignition in the process, and no gas was evolved.

The compound produced in other experiments of this kind was examined. It is a conductor of electricity, is of a dense

black, inflames spontaneously, and burns with a deep red light in the atmosphere.*

The non-conducting nature of the diamond and its infusibility, rendered it impossible to act upon it by Voltaic electricity; and the only new agents which seemed to offer any means of decomposing it, were the metals of the alkalies.

When a diamond is heated in a green glass tube with potassium, there is no elastic fluid given out, and no intensity of action; but the diamond soon blackens, and scales seem to detach themselves from it, and these scales, when examined in the magnifier, are gray externally, and of the colour of plumbago internally, as if they consisted of plumbago covered by the gray oxide of potassium.

In heating together three grains of diamonds in powder, and two grains of potassium, for an hour in a small retort of plate glass filled with hydrogene, and making the comparative trial with two grains of potassium heated in a similar apparatus, without any diamonds, I found that the potassium which had been heated with the diamonds, produced, by its action upon water, one cubical inch and $\frac{3}{10}$ of inflammable air, and that which had been exposed to heat alone, all other circumstances being similar, evolved nearly one cubical inch and $\frac{7}{10}$, both of which were pure hydrogene.

In another experiment of a similar kind, in which fragments of diamonds were used in the quantity of four grains, the potassium became extremely black from its action upon them during an exposure to heat for three hours, and the diamonds

* In the Bakerian Lecture for 1807, I have mentioned the decomposition of carbonic acid by potassium, which takes place with inflammation. If the potassium is in excess in this experiment, the same pyrophorus as that described above is formed.

were covered with a grayish crust, and when acted upon by water and dried, were found to have lost about $\frac{28}{100}$ of a grain in weight. The matter separated by washing, and examined, appeared as a fine powder of a dense black colour. When a surface of platina wire was covered with it, and made to touch another wire in the VOLTAIC circuit, a brilliant spark with combustion occurred. It burnt, when heated to redness in a green glass tube filled with oxygene gas, and produced carbonic acid by its combustion.

These general results seem to shew, that in plumbago the carbonaceous element exists merely in combination with iron, and in a form which may be regarded as approaching to that of a metal in its nature, being conducting in a high degree, opaque, and possessing considerable lustre.

Charcoal appears to contain a minute quantity of hydrogene in combination. Possibly likewise, the alkalies and earths produced during its combustion, exist in it not fully combined with oxygene, and according to these ideas, it is a very compounded substance, though in the main it consists of the pure carbonaceous element.

The experiments on the diamond render it extremely likely that it contains oxygene; but the quantity must be exceedingly minute, though probably sufficient to render the compound non-conducting: and if the carbonaceous element in charcoal and the diamond be considered as united to still less foreign matter in quantity, than in plumbago, which contains about $\frac{1}{20}$ of iron, the results of their combustion, as examined independently of hygrometrical tests, will not differ perceptibly.

Whoever considers the difference between iron and steel, in which there does not exist more than $\frac{1}{200}$ of plumbago, or the

difference between the amalgam of ammonium, and mercury, in which the quantity of new matter is not more than $\frac{1}{12000}$, or that between the metals and their sub-oxides, some of which contain less than $\frac{1}{10}$ of oxygene, will not be disposed to question the principle, that minute differences in chemical composition may produce great differences in external and physical characters.

6. *Experiments on the Decomposition, and Composition of the Boracic Acid.*

In the last Bakerian Lecture,* I have given an account of an experiment in which boracic acid appeared to be decomposed by VOLTAIC electricity, a dark coloured inflammable substance separating from it on the negative surface.

In the course of the spring and summer, I made many attempts to collect quantities of this substance for minute examination. When boracic acid, moistened with water, was exposed between two surfaces of platina, acted on by the full power of the battery of five hundred, an olive-brown matter immediately began to form on the negative surface, which gradually increased in thickness, and at last appeared almost black. It was permanent in water, but soluble with effervescence in warm nitrous acid. When heated to redness upon the platina it burnt slowly, and gave off white fumes, which slightly reddened moistened litmus paper, and it left a black mass, which, when examined by the magnifier, appeared vitreous at the surface, and evidently contained a fixed acid.

These circumstances seemed distinctly to shew the decomposition, and recomposition of the boracic acid; but as the

* Page 43.

peculiar combustible substance was a non-conductor of electricity, I was never able to obtain it, except in very thin films upon the platina. It was not possible to examine its properties minutely, or to determine its precise nature, or whether it was the pure boracic basis; I consequently endeavoured to apply other methods of decomposition, and to find other more unequivocal evidences upon this important chemical subject.

I have already laid before the Society an account of an experiment,* in which boracic acid, heated in contact with potassium in a gold tube, was converted into borate of potash, at the same time that a dark coloured matter, similar to that produced from the acid by electricity, was formed. About two months after this experiment had been made, namely, in the beginning of August, at a time that I was repeating the process, and examining minutely the results, I was informed, by a letter from Mr. CADELL at Paris, that M. THENARD was employed in the decomposition of the boracic acid by potassium, and that he had heated the two substances together in a copper tube, and had obtained borate of potash, and a peculiar matter concerning the nature of which no details were given in the communication.

That the same results must be obtained by the same methods of operating, there could be no doubt. The evidences for the decomposition of the boracic acid are easily gained, the synthetical proofs of its nature involve more complicated circumstances.

I found that when equal weights of potassium and boracic acid were heated together in a green glass tube, which had been exhausted after having been twice filled with hydro-

* Phil. Trans. Part II. 1808. p. 343.

gene, there was a most intense ignition before the temperature was nearly raised to the red heat; the potassium entered into vivid inflammation, where it was in contact with the boracic acid. When this acid had been heated to whiteness, before it was introduced into the tube, and powdered and made use of whilst yet warm, the quantity of gas given out in the operation did not exceed twice the volume of the acid, and was hydrogene.

I could only use twelve or fourteen grains of each of the two substances in this mode of conducting the experiment; for when larger quantities were employed, the glass tube always ran into fusion from the intensity of the heat produced during the action.

When the film of naphtha had not been carefully removed from the potassium, the mass appeared black throughout; but when this had been the case, the colour was of a dark olive-brown.

In several experiments, in which I used equal parts of the acid and metal, I found that there was always a great quantity of the former in the residuum, and by various trials, I ascertained that twenty grains of potassium had their inflammability entirely destroyed by about eight grains of boracic acid.

For collecting considerable portions of the matters formed in the process, I used metallic tubes furnished with stop-cocks, and exhausted after being filled with hydrogene.

When tubes of brass or copper were employed, the heat was only raised to a dull red; but when iron tubes were used, it was pushed to whiteness. In all cases the acid was decomposed, and the products were scarcely different.

When the result was taken out of a tube of brass or copper,

it appeared as an olive coloured glass, having opaque, dull olive-brown specks diffused through it.

It gave a very slight effervescence with water, and partially dissolved in hot water, a dark olive coloured powder separating from it.

The results from the iron tube, which had been much more strongly heated, were dark olive in some parts, and almost black in others. They did not effervesce with warm water, but were rapidly acted upon by it, and the particles separated by washing, were of a shade of olive, so dark as to appear almost black on white paper.

The solutions obtained, when passed through a filter, had a faint olive tint, and contained sub-borate of potash, and potash. In cases, when instead of water, a weak solution of muriatic acid was used for separating the saline matter, from the inflammable matter, the fluid came through the filter colourless.

In describing the properties of the new inflammable substance separated by washing, I shall speak of that collected from operations conducted in tubes of brass, in the manner that has been just mentioned; for it is in this way, that I have collected the largest quantities.

It appears as a pulverulent mass of the darkest shades of olive. It is perfectly opaque. It is very friable, and its powder does not scratch glass. It is a non-conductor of electricity.

When it has been dried only at 100 or 120°, it gives off moisture by increase of temperature, and if heated in the atmosphere, takes fire at a temperature below the boiling point of olive oil, and burns with a red light and scintillations like charcoal.

If it be excluded from air and heated to whiteness in a tube of platina, exhausted after having been filled with hydrogene, it is found very little altered after the process. Its colour is a little darker, and it is rather denser; but no indications are given of any part of it having undergone fusion, volatilization, or decomposition. Before the process its specific gravity is such that it does not sink in sulphuric acid; but after, it rapidly falls to the bottom in this fluid.

The phenomena of its combustion, are best witnessed in a retort filled with oxygene gas. When the bottom of the retort is gently heated by a spirit lamp, it throws off most vivid scintillations like those from the combustion of the bark of charcoal, and the mass burns with a brilliant light. A sublimate rises from it, which is boracic acid; and it becomes coated with a vitreous substance, which proves likewise to be boracic acid; and after this has been washed off, the residuum appears perfectly black, and requires a higher temperature for its inflammation than the olive coloured substance; and by its inflammation produces a fresh portion of boracic acid.

In oxymuriatic acid gas, the peculiar inflammable substance occasions some beautiful phenomena. When this gas is brought in contact with it at common temperatures, it instantly takes fire and burns with a brilliant white light, a white substance coats the interior of the vessel in which the experiment is made, and the peculiar substance is found covered by a white film, which by washing affords boracic acid, and leaves a black matter, which is not spontaneously inflammable in a fresh portion of the gas; but which inflames in it by a gentle heat, and produces boracic acid.

The peculiar inflammable substance, when heated nearly to redness in hydrogene, or nitrogene, did not seem to dissolve in these gasses, or to act upon them; it merely gained a darker shade of colour, and a little moisture rose from it, which condensed in the neck of the retort in which the experiment was made.

On the fluid menstrua containing oxygene, it produced effects which might be looked for from the phenomena of its agency on gasses.

When thrown into concentrated nitric acid, it rendered it bright red, so that nitrous gas was produced and absorbed, but it did not dissolve rapidly till the acid was heated; when there was a considerable effervescence, the peculiar substance disappeared, nitrous gas was evolved, and the fluid afforded boracic acid.

It did not act upon concentrated sulphuric acid till heat was applied; it then produced a slight effervescence; the acid became black at its points of contact with the solid; and a deep brown solution was formed, which, when neutralized by potash, gave a black precipitate.

When heated in a strong solution of muriatic acid, it gave it a faint tint of green; but there was no vividness of action, or considerable solution.

On acetic acid heated, it had no perceptible action.

It combined with the fixed alkalies, both by fusion and aqueous solution, and formed pale olive coloured compounds, which gave dark precipitates when decomposed by muriatic acid.

When it was kept long in contact with sulphur in fusion, it slowly dissolved, and the sulphur acquired an olive tint. It

was still less acted upon by phosphorus, and after an hour's exposure to it, had scarcely diminished in quantity, but the phosphorus had gained a tint of pale green.

It did not combine with mercury, when they were heated together.

These circumstances are sufficient to shew, that the combustible substance obtained from boracic acid by the agency of potassium, is different from any other known species of matter, and it seems, as far as the evidence extends, to be the same as that procured from it by electricity; and the two series of facts, seem fully to establish the decomposition, and recomposition of the acid.

From the large quantity of potassium required to decompose a small quantity of the acid, it is evident that the boracic acid must contain a considerable proportion of oxygene, I have endeavoured to determine the relative weights of the peculiar inflammable matter and oxygene, which compose a given weight of boracic acid; and to this end I made several analytical and synthetical experiments; I shall give the results of the two which I consider as most accurate.

Twenty grains of boracic acid and thirty grains of potassium, were made to act upon each other by heat in a tube of brass; the result did not effervesce when washed with diluted muriatic acid; and there were obtained after the process, by slight lixiviation in warm water, two grains and about $\frac{6}{16}$ of the olive coloured matter. Now thirty grains of potassium, would require about five grains of oxygene, to form thirty-five of potash; and according to this estimation, boracic acid must consist of about one of the peculiar inflammable substance, to nearly two of oxygene.

A grain of the inflammable substance in very fine powder, and diffused over a large surface, was set fire to in a retort, containing twelve cubical inches of oxygene; three cubical inches of gas were absorbed, and the black residuum collected after the boracic acid had been dissolved, was found to equal five eighths of a grain. This, by a second combustion, was almost entirely converted into boracic acid, with the absorption of two cubical inches and one eighth more of oxygene. The thermometer in this experiment was at 58° FAHRENHEIT, and the barometer at 30.2.

According to this result, boracic acid would consist of one of the inflammable matter, to about 1.8 of oxygene; and the dark residual substance, supposing it to be simply the inflammable matter combined with less oxygene than is sufficient to constitute boracic acid, would be an oxide, consisting of about 4.7 of inflammable matter, to 1.55 of oxygene.

These estimations, I do not however venture to give, as entirely correct. In the analytical experiments, there are probably sources of error, from the solution of a part of the inflammable matter, and it possibly may retain alkali, which cannot be separated by the acid. In the synthetical process, in which washing is employed, and so small a quantity of matter used, the results are still less to be depended upon; they must be considered only as imperfect approximations.

From the general tenour of the facts, it appears that the combustible matter obtained from boracic acid, bears the same relation to that substance, as sulphur and phosphorus do to the sulphuric and phosphoric acids. But is it an elementary

inflammable body, the pure basis of the acid? or is it not like sulphur and phosphorus, compounded?

Without entering into any discussion concerning ultimate elementary matter, there are many circumstances which favour the idea, that the dark olive substance, is not a simple body; its being non-conducting, its change of colour by being heated in hydrogene gas, and its power of combining with the alkalies; for these properties in general belong to primary compounds, that are known to contain oxygene.

I heated the olive coloured substance with potassium, there was a combination, but without any luminous appearance, and a gray metallic mass was formed; but from the effect of this upon water, I could not affirm that any oxygene had been added to the metal, the gas given off had a peculiar smell, and took up more oxygene by detonation than pure hydrogene, from which it seems probable, that it held some of the combustible matter in solution.

It occurred to me, that if the pure inflammable basis were capable of being deoxygenated by potassium, it would probably possess a stronger affinity for oxygene, than hydrogene, and therefore be again brought to its former state by water. I made another experiment on the operation of potassium, on the olive coloured substance, and exposed the mixture to a small quantity of ether, hoping that this might contain only water enough to oxygenate the potassium; but the same result occurred as in the last case; and a combination of potash and the olive coloured substance was produced, insoluble in ether.

I covered a small globule of potassium, with four or five times its weight of the olive coloured matter, in a platina tube

exhausted, after being filled with hydrogene; and heated the mixture to whiteness: no gas was evolved. When the tube was cooled, naphtha was poured into it, and the result examined under naphtha. Its colour was of a dense black. It had a lustre scarcely inferior to that of plumbago. It was a conductor of electricity. A portion of it thrown into water, occasioned a slight effervescence; and the solid matter separated, appeared dark olive, and the water became slightly alkaline. Another portion examined, after being exposed to air for a few minutes, had lost its conducting power, was brown on the surface, and no longer produced an effervescence in water.

Some of the olive inflammable matter, with a little potassium, was heated to whiteness, covered with iron filings, a dark metalline mass was formed, which conducted electricity, and which produced a very slight effervescence in water, and gave by solution in nitric acid, oxide of iron and boracic acid.

The substance which enters into alloy with potassium, and with iron, I am inclined to consider, as the true basis of the boracic acid.

In the olive coloured matter, this basis seems to exist in union with a little oxygene; and when the olive coloured substance is dried at common temperatures, it likewise contains water.

In the black non-conducting matter, produced in the combustion of the olive coloured substance, the basis is evidently combined with much more oxygene, and in its full state of oxygenation, it consitutes boracic acid.

From the colour of the oxides, their solubility in alkalies,

and from their general powers of combination, and from the conducting nature and lustre of the matter produced by the action of a small quantity of potassium upon the olive coloured substance, and from all analogy; there is strong reason to consider the boracic basis as metallic in its nature, and I venture to propose for it the name of *boracium*.

7. *Analytical Inquiries respecting Fluoric Acid.*

I have already laid before the Society, the account of my first experiments on the action of potassium, on fluoric acid gas.*

I stated, that the metal burns when heated in this elastic fluid, and that there is a great absorption of the gas.

Since the time that this communication was made, I have carried on various processes, with the view of ascertaining, accurately, the products of combustion, and I shall now describe their results.

When fluoric acid gas, that has been procured in contact with glass, is introduced into a plate glass retort, exhausted after being filled with hydrogene gas, white fumes are immediately perceived. The metal loses its splendour, and becomes covered with a grayish crust.

When the bottom of the retort is gently heated, the fumes become more copious; they continue for some time to be emitted, but at last cease altogether.

* Phil. Trans. Part II. 1808, p. 343. The combustion of potassium in fluoric acid, I have since seen mentioned in the number of the Moniteur, already so often quoted, as observed by M. M. Gay Lussac and Thenard; but no notice is taken of the results.

If the gas is examined at this time, its volume is found to be a little increased, by the addition of a small quantity of hydrogene.

No new fumes are produced by a second application of a low heat; but when the temperature is raised nearly to the point of sublimation of potassium, the metal rises through the crust, becomes first of a copper colour and then of a bluish black, and soon after inflames and burns with a most brilliant red light.

After this combustion, either the whole or a part of the fluoric acid, according as the quantity of potassium is great or small, is found to be destroyed or absorbed. A mass of a chocolate colour remains in the bottom of the retort; and a sublimate, in some parts chocolate, and in others yellow, is found round the sides, and at the top of the retort.

When the residual gas afforded by this operation, is washed with water, and exposed to the action of an electrical spark mixed with oxygene gas, it detonates and affords a diminution, such as might be expected from hydrogene gas.

The proportional quantity of this elastic fluid, differs a little in different operations. When the fluoric acid has not been artificially dried, it amounts to one sixth or one seventh of the volume of the acid gas used; but when the fluoric acid has been long exposed to calcined sulphate of soda, it seldom amounts to one tenth.

I have endeavoured to collect large quantities of the chocolate coloured substance for minute examination; but some difficulties occurred.

When I used from eighteen to twenty grains of potassium, in a retort containing from twenty to thirty cubical inches of

fluoric acid gas, the intensity of the heat was such, as to fuse the bottom of the retort, and destroy the results.

In a very thick plate glass retort, containing about nineteen cubical inches of gas, I once succeeded in making a decisive experiment on ten grains and a half of potassium, and I found that about fourteen cubical inches of fluoric acid disappeared, and about two and a quarter of hydrogene gas were evolved. The barometer stood at 30.3, and the thermometer at 61° FAHRENHEIT; the gas had not been artifically dried. In this experiment there was very little sublimate; but the whole of the bottom of the retort was covered with a brown crust, and near the point of contact with the bottom, the substance was darker coloured, and approaching in its tint to black.

When the product was examined by a magnifier, it evidently appeared consisting of different kinds of matter; a blackish substance, a white, apparently saline substance, and a substance having different shades of brown and fawn colour.

The mass did not conduct electricity, and none of its parts could be separated, so as to be examined as to this property.

When a portion of it was thrown into water, it effervesced violently, and the gas evolved had some resemblance in smell to phosphuretted hydrogene, and was inflammable.

When a part of the mass was heated in contact with air, it burnt slowly, lost its brown colour, and became a white saline mass.

When heated in oxygene gas, in a retort of plate glass, it absorbed a portion of oxygene, but burnt with difficulty, and required to be heated nearly to redness; and the light given

out was similar to that produced by the combustion of liver of sulphur.

The water which had acted upon a portion of it was examined; a number of chocolate coloured particles floated in it. When the solid matter was separated by the filter, the fluid was found to contain fluate of potash, and potash. The solid residuum was heated in a small glass retort in oxygene gas; it burnt before it had attained a red heat, and became white. In this process, oxygene was absorbed, and acid matter produced. The remainder possessed the properties of the substance formed from fluoric acid gas holding siliceous earth in solution, by the action of water.

In experiments made upon the combustion of quantities of potassium equal to from six to eleven grains, the portion of matter separable from the water has amounted to a very small part of a grain only, and operating upon so minute a scale, I have not been able to gain fully decided evidence, that the inflammable part of it is the pure basis of the fluoric acid; but with respect to the decomposition of this body by potassium, and the existence of its basis at least combined with a smaller proportion of oxygene in the solid product generated, and the regeneration of the acid by the ignition of this product in oxygene gas, it is scarcely possible to entertain a doubt.

The decomposition of the fluoric acid by potassium, seems analogous to that of the acids of sulphur and phosphorus. In neither of these cases are the pure bases, or even the bases in their common form evolved; but new compounds result, and in one case sulphurets, and sulphites, and in the other phosphurets, and phosphites of potash, are generated.

As silex was always obtained during the combustion of the

chocolate coloured substance obtained by lixiviation, it occurred to me that this matter might be a result of the operation, and that the chocolate substance might be a compound of the siliceous and fluoric bases in a low state of oxygenation, with potash; and this idea is favoured by some trials that I made to separate silex from the mass, by boiling it in concentrated fluoric acid; the substance did not seem to be much altered by this process, and still gave silex by combustion.

I endeavoured to decompose fluoric acid gas in a perfectly dry state, and which contained no siliceous earth; and for this purpose I made a mixture of one hundred grains of dry boracic acid, and two hundred grains of fluor spar, and placed them in the bottom of an iron tube, having a stop-cock and a tube of safety attached to it.

The tube was inserted horizontally in a forge, and twenty grains of potassium, in a proper iron tray, introduced into that part of it where the heat was only suffered to rise to dull redness. The bottom of the tube was heated to whiteness, and the acid acted upon by the heated potassium, as it was generated. After the process was finished, the result in the tray was examined.

It was in some parts black, and in others of a dark brown. It did not effervesce with water: and when lixiviated, afforded a dark brown combustible mass, which did not conduct electricity, and which when burnt in oxygene gas, afforded boracic, and fluoric acid. It dissolved with violent effervescence in nitric acid; but did not inflame spontaneously in oxymuriatic acid gas.

I have not as yet examined any of the other properties of

this substance; but I am inclined to consider it as a compound of the olive coloured oxide of boracium, and an oxide of the fluoric basis.

In examining the dry fluoric acid gas, procured in a process similar to that which has been just described, it gave very evident marks of the presence of boracic acid.

As the chocolate coloured substance is permanent in water, it occurred to me that it might possibly be producible from concentrated liquid fluoric acid at the negative surface in the VOLTAIC circuit.

I made the experiment with platina surfaces, from a battery of two hundred and fifty plates of six inches, on fluoric acid the densest that could be obtained by the distillation of fluor spar and concentrated sulphuric acid of commerce, in vessels of lead. Oxygene and hydrogene were evolved, and a dark brown matter separated at the deoxydating surface; but the result of an operation conducted for many hours, merely enabled me to ascertain that it was combustible, and produced acid matter in combustion; but I cannot venture to draw the conclusion that this acid was fluoric acid, as it was not impossible that some sulphureous, or sulphuric acid might likewise exist in the solution.

I heated the olive coloured inflammable substance, obtained from the boracic acid, in common fluoric acid gas in a plate glass retort; the temperature was raised till the glass began to fuse; but no change, indicating a decomposition, took place.

I heated six grains of potassium with four grains of powdered fluor spar in a green glass tube filled with hydrogene; there was a slight ignition, a minute quantity of hydrogene

gas was evolved, and a dark gray mass was produced, which acted upon water with much effervescence, but left no solid inflammable residuum.

8. *Analytical Experiments on Muriatic Acid.*

I have made a greater number of experiments upon this substance, than upon any of the other subjects of research that have been mentioned; it will be impossible to give any more than a general view of them within the limits of the Bakerian lecture.

Researches carried on some years ago, and which are detailed in the Journals of the Royal Institution, shewed that there were little hopes of decomposing muriatic acid, in its common form, by VOLTAIC electricity. When aqueous solution of muriatic acid is acted upon, the water alone is decomposed; and the VOLTAIC electrization of the gas affords no indications of its decomposition; and merely seems to shew, that this elastic fluid contains much more water than has been usually suspected.

I have already laid before the Society, an account of some experiments made on the action of potassium on muriatic acid. I have since carried on the same processes on a larger scale, but with precisely similar results.

When potassium is introduced into muriatic acid gas, procured from muriate of ammonia and concentrated sulphuric acid, and freed from as much moisture as muriate of lime is capable of attracting from it, it immediately becomes covered with a white crust, it heats spontaneously, and by the assistance of a lamp, acquires in some parts the temperature of ignition, but does not inflame. When the potassium and the gas are in

proper proportions, they both entirely disappear; a white salt is formed, and a quantity of pure hydrogene gas evolved, which equals about one third of the original volume of the gas.

By eight grains of potassium employed in this way, I effected the absorption of nearly twenty two cubical inches of muriatic acid gas; and the quantity of hydrogene gas produced was equal to more than eight cubical inches.

The correspondence between the quantity of hydrogene generated in cases of this kind, and by the action of potassium upon water, combined with the effects of ignited charcoal upon muriatic acid gas, by which a quantity of inflammable gas is produced equal to more than one third of its volume; seemed to shew, that the phenomena merely depended upon moisture combined with the muriatic acid gas.*

To determine this point with more certainty however, and to ascertain whether or no the appearance of the hydrogene was wholly unconnected with the decomposition of the acid, I made two comparative experiments on the quantity of muriate of silver, furnished by two equal quantities of muriatic acid, one of which had been converted into muriate of potash by the action of potassium, and the other of which had been absorbed by water; every care was taken to avoid sources of error; and it was found that there was no notable difference in the weight of the results.

* When the VOLTAIC spark is taken continuously, by means of points of charcoal in muriatic acid gas over mercury, muriate of mercury is rapidly formed, a volume of inflammable gas, equal to one third of the original volume of the muriatic acid gas appears. The acid gas enters into combination with the oxide of mercury, so that water enough is present in the experiment to form oxide sufficient to absorb the whole of the acid.

There was no proof then, that the muriatic acid had been decompounded in these experiments; and there was every reason to consider it as containing in its common aeriform state, at least one third of its weight of water; and this conclusion we shall find warranted by facts, which are immediately to follow.

I now made a number of experiments, with the hopes of obtaining the muriatic acid free from water.

I first heated to whiteness, in a well luted porcelain retort, a mixture of dry sulphate of iron, and muriate of lime which had been previously ignited; but a few cubic inches of gas only, were obtained, though the mixture was in the quantity of several ounces; and this gas contained sulphureous acid. I heated dry muriate of lime, mixed both with phosphoric glass and dry boracic acid, in tubes of porcelain, and of iron, and employed the blast of an excellent forge; but by neither of these methods was any gas obtained, though when a little moisture was added to the mixtures, muriatic acid was develloped in such quantities, as almost to produce explosions.

The fuming muriate of tin, *the liquor of Libavius*, is known to contain dry muriatic acid. I attempted to separate the acid from this substance, by distilling it with sulphur and with phosphorus; but without success. I obtained only triple compounds, in physical characters, something like the solutions of phosphorus, and sulphur in oil, which were non-conductors of electricity, which did not redden dry litmus paper, and which evolved muriatic acid gas with great violence, heat, and ebullition on the contact of water.

I distilled mixtures of corrosive sublimate and sulphur, and of calomel and sulphur; when these were used in their

common states, muriatic acid gas was evolved; but when they were dried by a gentle heat, the quantity was exceedingly diminished, and the little gas that was generated gave hydrogene by the action of potassium. During the distillation of corrosive sublimate and sulphur, a very small quantity of a limpid fluid passed over. When examined by transmitted light, it appeared yellowish green. It emitted fumes of muriatic acid, did not redden dry litmus paper, and deposited sulphur by the action of water. I am inclined to consider it as a modification of the substance discovered by Dr. THOMSON, in his experiments on the action of oxymuriatic acid on sulphur.

M. M. GAY LUSSAC and THENARD * have mentioned, that they endeavoured to procure dry muriatic acid by distilling a mixture of calomel and phosphorus, and that they obtained a fluid which they consider as a compound of muriatic acid, phosphorus, and oxygene. In distilling corrosive sublimate with phosphorus, I had a similar result, and I obtained the substance in much larger quantities, than by the distillation of phosphorus with calomel.

As oxymuriatic acid is slightly soluble in water, there was reason to suppose, reciprocally that water must be slightly soluble in this gas; I endeavoured therefore to procure dry muriatic acid, by absorbing the oxygene from oxymuriatic acid gas by substances, which when oxygenated, produce compounds possessing a strong affinity for water. Phosphorus, it is well known, burns in oxymuriatic acid gas; though the results of this combustion, I believe, have never been minutely examined. With the hopes of procuring muriatic acid gas, free from moisture, I made the experiment.

* The Moniteur before quoted.

I introduced phosphorus into a receiver having a stop-cock, which had been exhausted, and admitted oxymuriatic acid gas. As soon as the retort was full, the phosphorus entered into combustion, throwing forth pale white flames. A white sublimate collected in the top of the retort, and a fluid as limpid as water, trickled down the sides of the neck. The gas seemed to be entirely absorbed, for when the stop-cock was opened, a fresh quantity of oxymuriatic acid, nearly as much as would have filled the retort, entered.

The same phenomenon of inflammation again took place, with similar results. Oxymuriatic acid gas was admitted till the whole of the phosphorus was consumed.

Minute experiments proved, that no gaseous muriatic acid had been evolved in this operation, and the muriatic acid was consequently to be looked for either in the white sublimate, or in the fluid which had formed in the neck of the retort.

The sublimate was in large portions, the fluid only in the quantity of a few drops. I collected by different processes, sufficient of both for examination.

The sublimate emitted fumes of muriatic acid when exposed to air. When brought in contact with water, it evolved muriatic acid gas, and left phosphoric acid, and muriatic acid, dissolved in the water. It was a non-conductor of electricity, and did not burn when heated; but sublimed when its temperature was about that of boiling water, leaving not the slightest residuum. I am inclined to regard it as a combination of phosphoric, and muriatic acid in their dry states.

The fluid was of a pale greenish yellow tint, and very limpid; when exposed to air, it rapidly disappeared, emitting dense

white fumes which had a strong smell differing a little from that of muriatic acid.

It reddened litmus paper in its common state, but had no effect upon litmus paper which had been well dried, and which was immediately dipped into it. It was a non-conductor of electricity. It heated when mixed with water, and evolved muriatic acid gas. I consider it as a compound of phosphorous acid, and muriatic acid, both free from water.*

Having failed in obtaining uncombined muriatic acid in this way, I performed a similar process with sulphur, but I was unable to cause it to inflame in oxymuriatic acid gas. When it was heated in it, it produced an orange coloured liquid, and yellow fumes passed into the neck of the retort, which condensed into a greenish yellow fluid. By repeatedly passing oxymuriatic acid through this fluid, and distilling it several times in the gas, I rendered it of a bright olive colour, and in this case it seemed to be a compound of dry sulphuric, and muriatic acid, holding in solution a very little sulphur. When it was heated in contact with sulphur, it rapidly dissolved it, and then became of a bright red colour, and when saturated with sulphur, of a pale golden colour.† No permanent aeriform fluid was evolved in any of these operations, and no muriatic gas appeared, unless moisture was introduced.

As there seemed little chance of procuring uncombined

* I attempted to obtain dry muriatic acid likewise from the phosphuretted muriatic acid of M. M. GAY LUSSAC and THENARD, by distilling it in retorts containing oxygene gas, and oxymuriatic acid gas. In the first case, the retort was shattered by the combustion of the phosphorus, with a violent explosion. In the second, compounds, similar to those described above, were formed.

† All these substances seem to be of the same nature as the singular compound, the sulphuretted muriatic acid, discovered by Dr. THOMSON, noticed in page 93.

muriatic acid, it was desirable to ascertain what would be the effects of potassium upon it in these singular compounds.

When potassium was introduced into the fluid, generated by the action of phosphorus on corrosive sublimate, at first it slightly effervesced, from the action of the liquid on the moist crust of potash surrounding it; but the metal soon appeared perfectly splendid, and swimming on the surface. I attempted to fuse it by heating the fluid, but it entered into ebullition at a temperature below that of the fusion of the potassium; indeed the mere heat of the hand was sufficient for the effect. On examining the potassium, I found that it was combined at the surface with phosphorus, and gave phosphuretted hydrogene by its operation upon water.

I endeavoured, by repeatedly distilling the fluid from potassium in a close vessel, to free it from phosphorus, and in this way I succeeded in depriving it of a considerable quantity of this substance

I introduced ten or twelve drops of the liquid, which had been thus treated, into a small plate glass retort, containing six grains of potassium; the retort was exhausted after having been twice filled with hydrogene, the liquid was made to boil, and the retort kept warm till the whole had disappeared as elastic vapour. The potassium was then heated by the point of a spirit lamp; it had scarcely melted, when it burst into a most brilliant flame, as splendid as that of phosphorus in oxygene gas, and the retort was destroyed by the rapidity of combustion.

In other trials made upon smaller quantities after various failures, I was at last able to obtain the results; there was no proof of the evolution of any permanent elastic fluid during

the operation. A solid mass remained of a greenish colour at the surface, but dark gray in the interior. It was extremely inflammable, and often burnt spontaneously when exposed to air; when thrown upon water, it produced a violent explosion, with a smell like that of phosphuretted hydrogene. In the residuum of its combustion there was found muriate of potash, and phosphate of potash.

I endeavoured to perform this experiment in an iron tube, hoping that if the muriatic acid was decomposed in the process, its inflammable element, potassium and phosphorus, might be separated from each other by a high degree of heat; but in the first part of the operation the action was so intense, as to produce a destruction of the apparatus, and the stop-cock was separated from the tube with a loud detonation.

I heated potassium in the vapour of the compound of muriatic and phosphoric acid; but in this case, the inflammation was still more intense, and in all the experiments that I have hitherto tried, the glass vessels have been either fused or broken; the solid residuum has however appeared to be of the same kind as that I have just described.

The results of the operation of the sulphuretted compounds containing muriatic acid free from water upon potassium, are still more extraordinary than those of the phosphuretted compounds.

When a piece of potassium is introduced into the substance that distils over during the action of heated sulphur upon oxymuriatic acid, it at first produces a slight effervescence, and if the volume of the potassium considerably exceeds that of the liquid, it soon explodes with a violent report, and a most intense light.

I have endeavoured to collect the results of this operation, by causing the explosion to take place in large exhausted plate glass retorts; but, except in a case in which I used only about a quarter of a grain, I never succeeded. Generally the retort, though connected with the air pump at the time, was broken into atoms; and the explosion produced by a grain of potassium, and an equal quantity of the fluid, has appeared to me considerably louder than that of a musket.

In the case in which I succeeded in exploding a quarter of a grain, it was not possible for me to ascertain if any gaseous matter was evolved; but a solid compound was formed of a very deep gray tint, which burnt, throwing off bright scintillations, when gently heated, which inflamed when touched with water, and gave most brilliant sparks, like those thrown off by iron in oxygene gas.

Its properties certainly differed from those of any compound of sulphur and potassium that I have seen: whether it contains the muriatic basis must however be still a matter of inquiry.

There is, however, much reason for supposing, that in the singular phenomena of inflammation and detonation that have been described, the muriatic acid cannot be entirely passive: and it does not seem unfair to infer, that the transfer of its oxygene and the production of a novel substance, are connected with such effects, and that the highly inflammable nature of the new compounds, partly depends upon this circumstance. I am still pursuing the inquiry, and I shall not fail immediately to communicate to the Society, such results as may appear to me worthy of their attention.

9. *Some general Observations, with Experiments.*

An experiment has been lately published, which appeared so immediately connected with the discussion entered into in the second section of this Paper, that I repeated it with much earnestness.

In Mr. NICHOLSON's Journal for December, Dr. WOODHOUSE has given an account of a process, in which the action of water caused the inflammation of a mixture of four parts of charcoal and one of pearlash that had been strongly ignited together, and the emission of ammonia from them. I thought it possible, that in this case a substance might be formed similar to the residuum described in page 50; but by cooling the mixture out of the contact of nitrogene, I found that no ammonia was formed; and this substance evidently owed its existence to the absorption of atmospherical air by the charcoal.*

The experiments that I have detailed on the acids, offer some new views with respect to the nature of acidity. That a compound of muriatic acid with oxide of tin or phosphorus should

* Potash or pearlash is easily decomposed by the combined attractions of charcoal and iron; but it is not decomposable by charcoal, or, when perfectly dry, by iron alone. Two combustible bodies seem to be required by their combined affinities for the effect; thus in the experiment with the gun barrel, iron and hydrogene are concerned. I consider HOMBERG's pyrophorus as a triple compound of potassium, sulphur, and charcoal; and in this ancient process, the potash is probably decomposed by two affinities. The substance is perfectly imitated by heating together ten parts of charcoal, two of potassium, and one of sulphur.

When I first shewed the production of potassium to Dr. WOLLASTON in October 1807, he stated, that this new fact induced him to conceive that the action of potash upon platina, was owing to the formation of potassium, and proposed it, as a matter of research, whether the alkali might not be decomposed by the joint action of platina and charcoal.

not redden vegetable blues, might be ascribed to a species of neutralization, by the oxide or inflammable body; but the same reasoning will not apply to the dry compounds which contain acid matter only, and which are precisely similar as to this quality. Let a piece of dry and warm litmus paper be moistened with the compound of muriatic and phosphorous acid, it perfectly retains its colour. Let it then be placed upon a piece of moistened litmus paper, it instantly becomes of a bright red, heats and devellopes muriatic acid gas.

All the fluid acids that contain water are excellent conductors of electricity, in the class called that of imperfect conductors; but the compounds to which I have just alluded, are non-conductors in the same degree as oils, with which they are perfectly miscible. When I first examined muriatic acid, in its combinations free from moisture, I had great hopes of decomposing them by electricity; but there was no action without contact of the wires, and the spark seemed to separate no one of their constituents, but only to render them gaseous. The circumstance likewise applies to the boracic acid, which is a good conductor as long as it contains water; but which, when freed from water and made fluid by heat, is then a non-conductor.

The alkalies and the earthy compounds, and the oxides, as dry as we can obtain them, though non-conductors when solid, are, on the contrary, all conductors when rendered fluid by heat.

When muriatic acid, existing in combination with phosphorous or phosphoric acid, is rendered gaseous by the action of water, the quantity of this fluid that disappears, at least equals from one third to two fifths of the weight of the acid gas

produced; a circumstance that agrees with the indications given by the action of potassium.*

I attempted to procure a compound of dry muriatic and carbonic acids, hoping that it might be gaseous, and that the two acids might be decomposable at the same time by potassium. The process that I employed was by passing corrosive sublimate in vapour through charcoal ignited to whiteness; but I obtained a very small quantity of gas, which seemed to be a mixture of common muriatic acid gas and carbonic acid gas; a very minute portion of running mercury only was obtained, by a long continuation of the process; and the slight decomposition that did take place, I am inclined to attribute to the production of water, by the action of the hydrogene of the charcoal upon the oxygene of the oxide of mercury.†

In mixing muriatic acid gas with carbonic acid, or oxygene, or hydrogene, the gases being in their common states, as to moisture, there was always a cloudiness produced; doubtless owing to the attraction of their water to form liquid muriatic acid.

On fluoric acid gas no such effect was occasioned. This fact, at first view, might be supposed to shew, that the hydrogene evolved by the action of potassium upon fluoric acid gas, is

* Page 98.

† These facts and the other facts of the same kind, explain the difficulty of the decomposition of the metallic muriates in common processes of metallurgy. They likewise explain other phenomena in the agencies of muriatic salts. In all cases when a muriatic salt is decomposed by an acid, and muriatic acid gas set free, there appears to be a double affinity, that of the acid for the basis, and of the muriatic acid for water; pure muriatic acid does not seem capable of being displaced by any other acid.

owing to water in actual combination with it, like that in muriatic acid gas, and which may be essential to its elastic state; but it is more probable, from the smallness of the quantity, and from the difference of the quantity in different cases, that the moisture is merely in that state of diffusion or solution in which it exists in gases in general, though from the disposition of water to be deposited in this acid gas in the form of an acid solution, it must be either less in quantity, or in a less free state, so as to require for its exhibition much more delicate hygrometrical tests.

The facts advanced in this Lecture, afford no new arguments in favour of an idea to which I referred in my last communication to the Society, that of hydrogene being a common principle in all inflammable bodies; and except in instances which are still under investigation, and concerning which no precise conclusions can as yet be drawn, the generalization of LAVOISIER happily applies to the explanation of all the new phenomena.

In proportion as progress is made towards the knowledge of pure combustible bases, so in proportion is the number of metallic substances increased; and it is probable that sulphur and phosphorus, could they be perfectly deprived of oxygene, would belong to this class of bodies. Possibly their pure elementary matter may be procured by distillation, at a high heat, from metallic alloys, in which they have been acted upon by sodium or potassium. I hope soon to be able to try this experiment.

As our inquiries at present stand, the great general division of natural bodies is into matter which is, or may be supposed to be, metallic, and oxygene; but till the problem concerning

the nature of nitrogene is fully solved, all systematic arrangements made upon this idea must be regarded as premature.

EXPLANATION OF THE FIGURES.

Fig. 1. The retort of plate glass for heating potassium in gases.

Fig. 2. The tray of platina for receiving the potassium.

Fig. 3. The platina tube for receiving the tray in experiments of distillation.

Fig. 4. The apparatus for taking the VOLTAIC spark in sulphur and phosphorus.

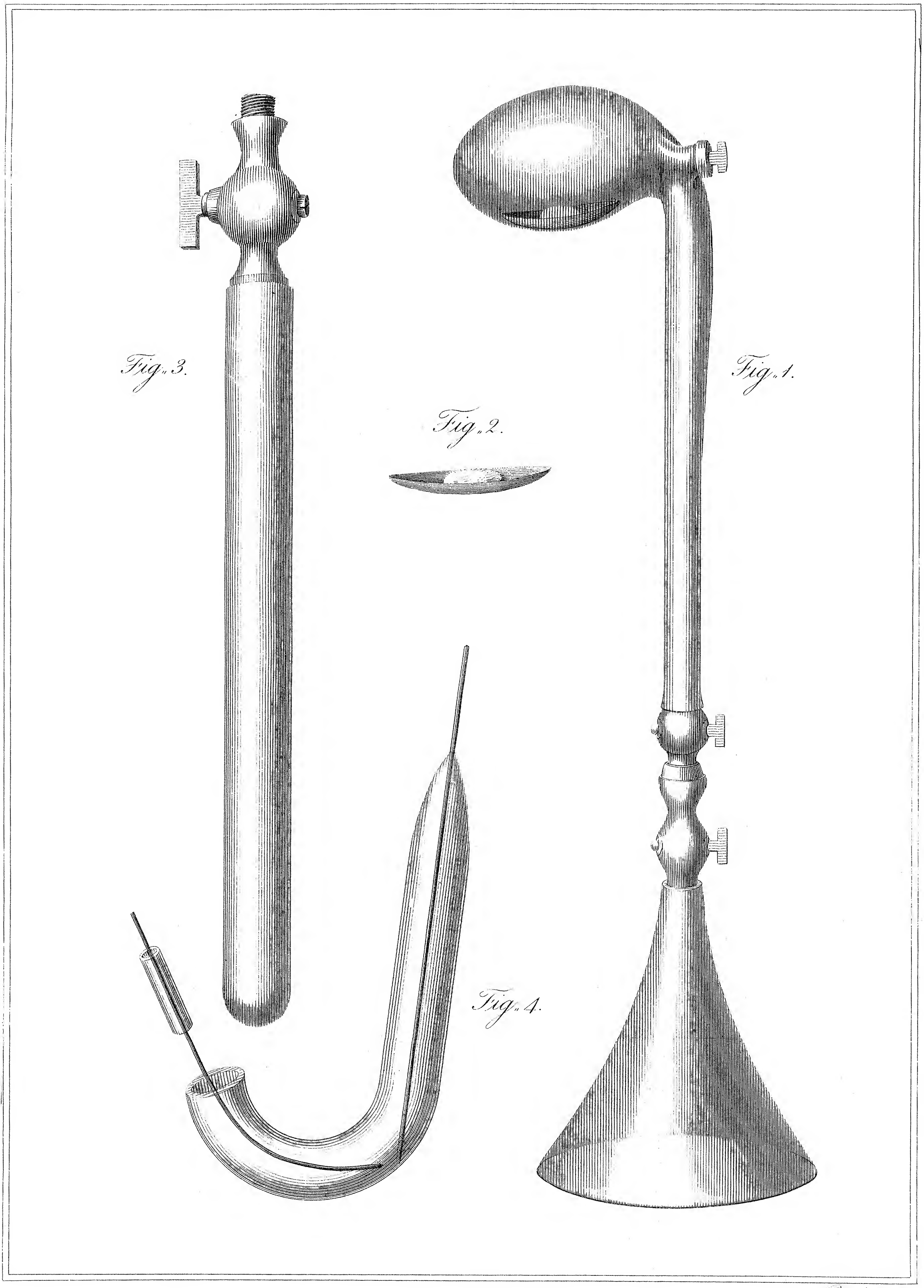

Fig. 3.

Fig. 1.

Fig. 2.

Fig. 4.

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